with small instruments what at common altitudes can be done only with large ones; and when possible it is always better to use small instruments, since they are more easily handled, and are relatively stronger and better than large ones. Uniformity of temperature may be secured by seeking locations in the tropical islands, or on the coasts like that of California, where the ocean winds keep the temperature nearly uniform throughout the year. At great altitudes we may secure a clearness of vision that would be of the greatest value in the examination of faint objects, and by this means, and by persevering and continuous observation, interesting discoveries may be made. It is a matter of course that, except in the case of comets, the future discoveries in astronomy will belong to faint and delicate objects; but these are interesting, and should not be neglected. A uniform temperature, which secures good definition, and steady images of the stars, is necessary for accurate determinations of position, and for all measurements of precision. This condition is especially important in such work as that of stellar parallax, the determination of the constant of aberration, and wherever the yearly change of temperature may act injuriously. In the selection of better sites for observatories, I think we have an easy means of advancing astronomy.

As this science grows and expands, it will become more and more necessary to study the economy of its work, in order that astronomers may bestow their labors in the most advantageous methods, and may rid themselves of all cumbersome and time-consuming processes. The manner of publishing observations has already been much abbreviated, and improved, I think, by some of the European astronomers, and this change seems destined to become universal. As the positions of many objects are now well known, the need of printing all the details of the observation, such as the transit of the wires, the readings of the micrometers, etc., is very slight; and this printing may be safely abandoned. Even this change will lead to a great saving in the time and cost of printing. But this will necessitate a more complete discussion of the work and a more careful examination of the instruments; things to be desired, since they tend to lift the observer out of his routine, and make him a master of his business. There are objections to this change, and some of them are real, such as the importance of publishing a complete record; but this is overestimated, I think, since the original records ought always to be referred to in case of doubt; and other objections are factitious, such as the need of publishing a large a showy book in order to impose on the public.

We may hope also for improvements in theoretical astronomy, and for the better training and preparation of students of this science. I know that it is sometimes said that theoretical astronomy is finished, and that nothing more can be done. Such assertions come from professors who are old and weary, or from those young men who tire out early in life; but they are wrong. The improvements that Hansen has made in the theory of perturbations, and Poinsot's study of the theory of rotation, show what careful investigation may do, and assure us of further progress. It must be confessed that some of the astronomical work done in our country bears evidence that the astronomers did not understand the correct methods of reduction, and much of it shows evidence of hasty and ill-considered plans. This is perhaps a natural condition for beginners, but we trust that it has been outgrown. An actual need for the astronomical students of our country is a good book on theoretical astronomy, similar to Pontécoulant's work, in which the whole subject shall be presented in a complete form, such as we find in the *Mécanique Céleste*, together with an account of the improvements made by Gauss, Poisson, Hansen and others. There is no American book of this kind, and the English works are too partial, designed apparently to fit the student for college examinations, and not to give him a complete knowledge of the science. Such a book has hardly been attempted in our language, unless that of Woodhouse may be an exception, and it may be a long time in coming, since it requires a man qualified to do the work, and will involve an expense of labor in the preparation, and of cost in publishing, such as few are willing to incur. In the meantime it is far better for the student to go directly to the writings of Lagrange and Laplace, of Gauss and Poisson and other masters, rather than to spend time in reading second-rate authors who endeavor to explain them. And generally this will be found the easier way also, since the student avoids the confused notions and symbols, and the grotesque expressions and egotism of small men, and is lifted into the region of ideas and invention.

In presenting his exposition of the nebular hypothesis, which has since become so celebrated, Laplace says: "I present this hypothesis with the distrust which everything ought to inspire that is not a result of observation or of calculation." It is a singular fact that, among all the writings on the nebular hypothesis, I have never seen a reference to this presentation of it by its most distinguished advocate; and yet this is the true spirit of scientific astronomy. Laplace did not wish to exempt his own theories from criticism, and neither should anyone. In astronomy there is no final human authority, no synod or council, but simply an appeal to reason and observation. If a theory or a discovery be true, it will stand the test of observation and of calculation; if false, it must pass away to that Miltonian limbo where so many things have gone and are going. The question is sometimes asked, of what use is astronomy? and the reply generally made is that it has conferred great benefits on navigation and on commerce, since it is by means of his astronomical knowledge that the sailor determines the position of his ship on the ocean. There is a truth in this reply, but it is only partial. The great value of astronomy is that it is really a science, and that it has broken the path and led the way through which all branches of science must past if they ever become scientific. It is the spirit of honest, unrelenting criticism, and of impartial examination, that finally eliminates error and awards to every one his just due, that makes astronomy honorable and attractive; and it is by cultivating this spirit that astronomy confers its chief benefit, for it is this that shall break in pieces and destroy all false assumptions in science and in philosophy.

JOSEPH HENRY.

EULOGY BY PROFESSOR A. M. MAYER.

At the meeting of the Association in 1878, a committee, composed of Professors Baird, Newcomb and myself, was appointed to prepare a eulogy on our revered and lamented colleague and former president, Joseph Henry. This—I will not say labor, but duty of affection—has devolved on me alone. I would that the other members of this committee had laid before you their tributes to his memory, because for years they had been closely associated with him in his social and professional life in Washington. Yet, while Professor Henry had been the friend of their manhood, he was the friend of my boyhood; and during 25 years he ever regarded me—as was his wont to say—with a "paternal interest." To his disinterested kindness and wise counsels is due much, very much, of whatever usefulness there is in me. Hence I have said that it is a duty of affection for me to speak to you about one who was my beloved friend. I shall not, however, attempt a biography of Joseph Henry, nor will I speak of his administrative life as director of the Smithsonian Institution, for this is known and valued by the whole world. His best eulogy is an account of his discoveries; for a man of science, *as such*, lives in what he has *done*, and not in what he has *said*; nor will he be remembered in what he proposed to do. I will, therefore, with your permission, confine myself chiefly to Henry *as the* discoverer; and I do this the more willingly because I am familiar with his researches, and also because Professor Henry, from time to time, took pleasure in giving me accounts of these mental conceptions which preceded his work, led him to it and guided him in it. Rightly to appreciate a discoverer, we should not look at his work from our time, but go back and regard it from his time; we should not judge his work in the fulness of the light of present knowledge, but in the dim twilight which alone illuminated him to then unknown—but now well-known—facts and laws. I will, therefore, endeavor first to present you with a clear, but necessarily very concise, view of the state of our knowledge of electricity when Henry began his original researches in that branch of science, and then point out the value of his discoveries, by showing that they added to knowledge, and how they instigated and influenced the discoveries and inventions of other men. Henry began his electrical researches at the age of twenty-eight, in the year 1827, while he was professor of mathematics and natural philosophy in the Albany Academy. At these he continuously worked till 1832, when, at

the age of thirty-three, he moved to Princeton College. After a year's break in his work, caused by the preparation of his course of lectures for the college, he is again at original research, and continues his contributions to electrical discoveries till 1842. Thus, during fourteen years, between the ages of twenty-eight and forty-three, he was a constant and fertile worker.

As with many other men of originality, Henry's first essays were in the direction of improving the means of illustrating well-established scientific facts and principles. His first paper of October, 1827, is interesting because it was his first. In it he improves on the usual apparatus which had been used by Ampère and others to show electro-dynamic actions, by employing several turns of insulated wire, instead of one, as had previously been the practice. Thus, for example, to show the directive action of the earth's magnetism on a freely-moving closed circuit, Henry covered copper wire with silk, and then made out of it a ring about twenty inches in diameter, formed of several turns of the wire. The extremities of this wire were soldered to zinc and copper plates. The coil was then suspended by silk filaments. On plunging the metal plates into a glass of dilute acid the ring rotated around its point of suspension till its plane took a permanent position at right angles to the magnetic meridian. By a similar arrangement of two concentric coils, one suspended within the other, he neatly showed the mutual actions of voltaic currents flowing in the same or opposite directions, which facts are the foundations of Ampère's celebrated law. We now reach a period when Henry appears as a discoverer, and truly one of no mean order. As I remember his narration to me in the year 1859, it was as follows: He said that one evening he was sitting in his study in Albany with a friend, when, after a few moments of reverie, he arose and exclaimed, "Tomorrow I shall make a capital experiment!" For several months he had been brooding over Ampère's electro-dynamic theory of magnetism, and he was then deeply interested in the phenomena of the development of magnetism in soft iron, as shown in the experiments of Arago and Sturgeon. At the moment he had arisen from his chair it had occurred to him that the requirements of the theory of Ampère were not fulfilled in the electro-magnets of Arago and of Sturgeon, but that he could get those conditions which the theory required by covering the developing wire with a non-conductor, like silk, and then wrapping it closely around the soft iron bar in several layers; for the successive layers of wire coiling first in one direction and then in the other would tend to produce a resultant action of the current at right angles to the axis of the bar; and furthermore, the great number of convolutions thus obtained would act on a greater number of molecules of the bar, and therefore exalt its magnetism. "When this conception," said Henry, "came into my brain, I was so pleased with it that I could not help rising to my feet and giving it my hearty approbation." Henry did go to work next day, and to his great delight and encouragement discoveries of the highest interest and importance revealed themselves to him week after week. When he had finished his newly conceived magnet he found that it supported several times more weight than did Sturgeon's magnet of equal size and weight. This was his first original discovery.

I will now give, as far as possible, Henry's own words in narrating the subsequent investigations of these very interesting phenomena: "The maximum effect, however, with this arrangement and a single battery was not yet obtained. After a certain length of wire had been coiled upon the iron the power diminished with a further increase of the number of turns. This was due to the increased resistance which the larger wire offered to the conduction of electricity. Two methods of improvement, therefore, suggested themselves. The first consisted, not in increasing the length of coil, but in using a number of separate coils on the same piece of iron. By this arrangement the resistance to the conduction of the electricity was diminished and a greater quantity made to circulate around the iron from the same battery. The second method of producing a similar result consisted in increasing the number of elements of the battery, or, in other words, the projectile force of the electricity, which enabled it to pass through an increased number of turns of wire, and thus, by increasing the length of the wire, to develop the maximum power of the iron. To test these principles on a larger scale an experimental magnet was constructed. In this a number of compound helices were placed on the same bar, their ends left projecting, and so numbered that they could be all united into one long helix, or variously combined in sets of lesser length. From a series of experiments with this and other magnets it was proved that, in order to produce the greatest amount of magnetism from a battery of a single cup, a number of helices is required; but when a compound battery is used, then one long wire must be employed, making many turns around the iron, the length of wire and consequently the number of turns being commensurate with the projectile power of the battery. In describing the results of my experiments the terms *intensity* and *quantity* magnets were introduced to avoid circumlocution, and were intended to be used merely in a technical sense. By the intensity magnet I designated a piece of soft iron so surrounded with wire that its magnetic power could be called into operation by an intensity battery; and by a quantity magnet a piece of iron so surrounded by a number of separate coils that its magnetism could be fully developed by a quantity battery. "I was," said Henry, "the first to point out this connection of the two kinds of the battery with the two forms of the magnet, in my paper in Silliman's Journal, January, 1831, and clearly to state that when magnetism was to be developed by means of a compound battery one large coil was to be employed, and when the maximum effect was to be produced by a single battery a number of strands were to be used."

We will now return to Henry's study of the properties of his intensity magnet. This magnet was formed of a piece of iron one-fourth of an inch in diameter, bent in the U form and wound with eight feet of insulated wire. His batteries were two,—one formed of a single element with a zinc plate four inches by seven, surrounded by copper and immersed in dilute acid; the other, a Cruikshank's battery, or trough, with twenty-five double plates. The plates of this battery were joined in series, and altogether had exactly the same surface of zinc as that in the single-cell battery. The magnet was now connected directly to the single cell. The magnet held up seventy-two ounces. Then five hundred and thirty feet of number 18 copper wire led the current from the cell to the magnet; it now supported only two ounces. Five hundred and thirty feet more of the wire were introduced into the circuit, and then the magnet held but one ounce. In these facts Henry faced the same results as confronted Barlow five years before, and caused Barlow then to say: "In a very early stage of electro-magnetic experiments it had been suggested [by Laplace, Ampère and others] that an instantaneous telegraph might be established by means of conducting wires and compasses, but I found such a sensible diminution with only two hundred feet of wire, as at once to convince me of the impracticability of the scheme"; and such, at that day, seemed to be the common opinion of men of science. But this opinion is presently to be shown by Henry to be ill-founded, by reason of the ignorance of the relations which have of necessity to exist between the kind of battery and the kind of magnet in order to produce electro-magnetic action at a distance—relations which Henry was the first to discover. This accomplishment justly entitles him to be regarded as a man of genius and a discoverer of no mean order. This discovery will always remain the one important fact that was to be known, to be understood, and to be applied, before it was possible to have constructed any form of electro-magnetic telegraph. Let us see how Henry made this discovery.

After ending the experiments with the one-cell battery and reaching results which seemed to confirm the opinion of Barlow as to the "impracticability of the scheme" of an electro-magnetic telegraph, Henry attached his magnet to the second battery formed of twenty-five cells, arranged in series. The current from this battery was sent to the magnet through 1060 feet of the same wire as had been used in the experiments with the first battery of one cell. The magnet now lifted eight ounces. It had held up only one ounce, when with the same length of interposed wire the battery of one cell was used. He now attached his electro-magnet directly to the poles of the 25-cell battery, when, to his astonishment, it only held seven ounces. The same magnet it will be remembered, when attached to the one-cell battery, supported seventy-two ounces. Here were facts of the highest significance, and Henry was not slow to seize them in all their bearings. Referring to these experiments, he said in 1857: "These steps in the advance of electro-magnetism, though small, were such as to interest and astonish the scientific world. These developments were considered at the time of much importance in a scientific point of view, and they subsequently furnished the means by which magneto-electricity, the phenomena of dia-magnetism, and the magnetic effects in polarized light were discovered. They gave rise to the various forms of electro-magnetic machines which have exercised the ingenuity of inventors in every part of the world, and were of immediate applicability in the introduction of the magnet to telegraphic purposes. Neither the electro-magnet of Sturgeon nor any electro-magnet ever made previous to my investigations was applicable to transmitting power to a distance."

Not satisfied with the mere statement that his discovery was "directly applicable to Mr. Barlow's project of forming an electro-magnetic telegraph," he actually constructed one, some time during the year 1831, around one of the upper rooms of the Albany Academy. It was more than a mile in length, and made signals by sounding a bell. This was the first electro-magnetic telegraph which had worked through so great a length of wire. It was the first "sounding" electro-magnetic telegraph. The relative parts played by Henry and Morse are described in Henry's "Statement" published by the Smithsonian in 1857. "*The principles*," says Henry, "*I had developed were applied by Dr. Gale to render Morse's machine effective at a distance.*" This statement seems to be as direct, as clear, as truthful, and as comprehensive as one can desire. I will take the liberty of remarking that had Henry taken out a patent in which he claimed as his invention an electro-magnet formed of two or more layers of insulated wire, Morse's patent would not have been so valuable. Remember, I speak not of the merit of the invention, but of the merit of the patent; for the invention, so far as Morse is concerned, would have remained the same, because one essential part of a Morse telegraph is Henry's intensity magnet, and certainly Morse never invented that.

If Ohm's law had been known to Henry, with all of its consequences, when applied to his discovery of the exaltation of the electro-magnetism of iron, in connection with his discovery of the

proper relations necessary between batteries and magnet to get the greatest electro-magnetic effects, his discoveries would appear dwarfed, though yet of excellent workmanship. But did he at this time, 1827 to 1832, know of Ohm's law? I infer that Henry arrived at his discoveries independently of such knowledge, and for two-fold reasons. First, Ohm's law was published as late as 1827, in Berlin, and was received almost contemptuously. Henry was unable to read German, and Ohm's papers were first published in English in 1841. Secondly, from the manner in which Henry worked at his problems and viewed his results, I conclude that he had no knowledge of Ohm's laws; else why should he have been astonished at the effects when his intensity magnet was connected with his intensity battery? Henry, now in possession of powerful magnets, began to work on another problem. He tried to do the reverse of what he had already done. His magnet was made by the action of the electric current, and he now tried to obtain an electric current from the magnet; and he succeeded. Henry and Faraday independently discovered the means of producing an electric current and spark from a magnet. Tyndall speaks of this experimental results as the "Mont Blanc of Faraday's own achievements." A few words now will place Henry in his proper and just relations to these important discoveries. All the information he had received about Faraday's discovery was the account of Faraday's production of magneto-electricity by the sudden insertion of a magnet into a helix and its sudden withdrawal therefrom. Henry's experiment is entirely different, and certainly was entirely original with him; but it is essentially identical with another of Faraday's of which Henry had no knowledge. Thus it appears that, although Henry cannot be placed on record as the *first* discoverer of the magneto-electric current, he stands alone as its *second* independent discoverer.

Henry's next discovery was that of the induction of a current upon itself, or of the extra current, as it is sometimes called. Here he anticipated Faraday by nearly two years and a half in the observation of the fundamental facts. Notwithstanding an explicit disclaimer of Faraday, the credit of this discovery has been generally given to the latter. This is accounted for by the fact that, although Henry anticipated others in his observations, he had not leisure to follow them up to their full explanation until after Faraday had completely unraveled their nature. In 1838, after his return from a first visit to Europe, Henry discovered an entirely new class of phenomena in electrical induction. He first showed that an induced current may excite a second induced current in a neighboring closed conductor, that this last may induce a third current, and so on. These currents Henry styled currents of the first, second, third, etc., orders, and he showed that they alternate in their direction successively. He investigates the difference in these currents as they flow through different resistances. The same phenomena he tracks through the inductive sections of the discharge of the Leyden jar and of the frictional electrical machine, and shows how they differ from those produced by the voltaic battery. These researches are the most finished of Henry's investigations, and will ever be regarded as models of careful and thorough scientific work.

Henry had a versatile mind, and did not confine his attention to the study of electricity. His researches in molecular physics, though not extensive, are remarkable. Here his suggestions and methods have stimulated others to follow in the paths which he has pointed out. In 1839 Henry made a curious discovery as to the permeability of lead to mercury. He found mercury would even ascend a lead wire to the height of a yard in a few days. He even made what might be called syphons of lead, which would nearly empty a vessel of mercury by drawing the fluid over its sides. Subsequently, in 1845, with Mr. Cornelius, he proved that copper, when heated to the melting point of silver, would absorb the latter metal. In 1844 Henry was investigating the nature of the forces acting in liquid films. Studying the tenacity of the soap-bubble film, although his experiments could only furnish approximate results, they showed that the molecular attraction of water for water is really several hundred pounds to the square inch, and probably equal to the attraction of ice for ice. Another of Henry's investigations, having a practical bearing, should be more widely known than it is. Among his duties as chairman of the United States lighthouse board was the testing of the various physical properties of the oils submitted to the government for purchase. Fluidity was one of these properties for which it seemed most difficult to get reliable tests. Here he very ingeniously applied the theorem of Torricelli, which shows that equal quantities of all liquids of equal fluidity will flow out of an orifice in equal times. Henry found that with different oils the flow of equal quantities differed, the rapidity of flow of sperm oil exceeding that of lard oil in the ratio of 100 to 167. Alcohol proved to be less fluid than water. Henry took a deep interest in acoustics. His additions to this science were chiefly the results of experiments upon fog signals. He made extensive experiments with various sound-producing instruments, and eventually decided in favor of the steam syren fog-horn. He determined that these instruments send their sound farthest when tuned very near to the treble C, and he also showed the uselessness of applying reflectors to them. During eleven years Henry sought to advance the efficiency of our fog signals by experiments in all weathers. Many very puzzling facts were collected. Thus it was observed that a sound coming to a mariner against the wind would cease to be audible on the deck of his vessel while it continued to be heard at the masthead. It was also observed that upon approaching a fog-horn from a distance the intensity of sound would gradually increase, then die down rapidly, become inaudible through a space of three or four miles, and perhaps not reappear until the vessel was within a mile of the instrument. These facts demanded explanations, and for a long time remained enigmas to Henry, till one day he met with a paper by Professor Stokes, in which the effect of an upper current in deflecting a wave of sound is fully explained. This hypothesis of Stokes Henry was able to apply to the solution of the problems in question.

Henry's services to the light-house board were of great value to the country. The fact that his investigations showed that lard oil heated to about 250° Fahrenheit is superior in fluidity and illuminating power to sperm oil caused the substitution of the former for the latter. A dollar a gallon was saved, which amounts to about one hundred thousand dollars a year in favor of the government. In light and heat Henry made several investigations which we must pass over. One, however, is so important that it cannot be omitted. I refer to his application of the thermopile in determining the distribution of heat on the optical images of distant objects. In a bold, and wonderful experiment, he sought to study the distribution of heat on the surface of the sun. In 1845, with Stephen Alexander, he formed an image of the sun, by means of a telescope, upon a screen. In this screen was cut an aperture, closed by the surface of a thermopile. By a motion of the telescope, any part of the image could be brought upon the pile. A solar spot being present, he clearly proved that it emitted less heat than the surrounding parts of the luminous disc. This method of research was shown to Secchi. On his return to Europe the latter made no small repute by extending these observations, using Henry's methods, but often, I fear, not giving full credit to the originator. But let that pass, for the bread which Henry cast upon the waters has returned to our own shores, thanks to the genius of our colleague Langley.

It is impossible to crowd into one brief hour the thoughts which were his occupation during more than half a century. I have at least endeavored to exhibit the more important part of the labors of his life. What shall we think of them? Surely they are on as high a plane as those of any of his contemporaries, and show as much originality as theirs in their conception—as much skill in their execution. Yet it has been said that Henry was not a man of genius. As I have not been able to find that the philosophers who have the special charge of giving from time to time definitions of genius, have been able to come to any satisfactory conclusion among themselves, I will leave their company, and, with your liberty, take my definition from a book which, if we accredit Thackeray, is one of the very best, if not the best, novel ever written in English. After listening to this I will allow you to form your own opinions as to whether Henry did or did not possess genius. "By genius I would understand that power, or rather those powers, of the mind which are capable of penetrating into all things within our reach and knowledge, and of distinguishing their essential differences. These are no other than invention and judgment, and they are both called by the collective name of genius, as they are of those gifts of nature which we bring with us into the world. Concerning each of which many seem to have fallen into very great errors; for by invention, I believe, is generally understood a creative faculty, which would indeed prove most romance writers to have the highest pretensions to it; whereas by invention is meant no more, and the word so signifies, than discovery in finding out; or, to explain it at large, a quick and sagacious penetration into the true essence of all the objects of our contemplation. This, I think, can rarely exist without the concomitancy of judgment, for how we can be said to have discovered the true essence of two things, without discovering their difference, seems to me hard to conceive. Now this last is the undisputed province of judgment; and yet some few men of wit have agreed with all the dull fellows in the world in representing these two to have seldom or never been the property of one and the same person." My own judgment, if of any value, would rank the ability of Henry—I do not say his achievements—a little below that of Faraday. Indeed their lives and their manners of working were strangely alike. Faraday was the son of a blacksmith. He once wrote: "I love a smith's shop and anything relating to smithery. My father was a smith." Henry's father plied a schooner on the Hudson. Each started in life with moral and benevolent habits, well-developed and healthy bodies, quick and accurate perceptions, calm judgment and self reliance, tempered with morality and good manners—a good ground, surely, in which to plant the germs of the scientific life. Faraday was an apprentice to a bookbinder. Henry served in the same capacity under a blacksmith. Each, endowed with a lively imagination, was in his younger days fond of romance and the drama; and, by a singular similarity of accidents, each had his attention turned to science by a book which chance threw in his way. This work in the case of Faraday was "Mrs. Marcet's Conversations on Chemistry," and the book which influenced Henry's career was "Gregory's Lectures on Experimental Philosophy, Astronomy and Chemistry." Of Mrs. Marcet's book Faraday thus writes:—"My Dear Friend, —Your subject interested me deeply every way; for Mrs. Marcet was a good friend to me, as she must have been to many of the human race. I entered the shop of a bookseller and bookbinder at

the age of thirteen, in the year 1804, remaining there eight years, and during the chief part of the time bound books. Now it was in those books, in the hours of the week, that I found the beginning of my philosophy. There were two that especially helped me,—the Encyclopœdia Britannica, from which I gained my first notions of electricity, and Mrs. Marcet's "Conversations on Chemistry," which gave me my foundation in that science. Do not suppose that I was a very deep thinker, or was marked a precocious person. I was a burly imaginative person, and could believe in the Arabian Nights as easily as in the Encyclopœdia. But facts were important to me and saved me. I could trust a fact and always cross-examined an assertion. So when I questioned Mrs. Marcet's book by such little experiments as I could find means to perform, and found it true to the facts as I could understand them, I felt that I had got hold of an anchor in chemical knowledge, and clung fast to it. Thence my deep veneration for Mrs. Marcet—first, as one who had conferred great personal good pleasure on me; and then as one able to convey the truth and principle of those boundless fields of knowledge which concern natural things to the young, untaught and inquiring mind. You may imagine my delight when I came to know Mrs. Marcet personally; how often have I cast my thoughts backward, delighting to connect the past and the present; how often, when sending her a paper as a thank-offering, I thought of my first instructress; and such thoughts will remain with me."

Henry wrote on the inside of the cover of Gregory's work the following words: "This book, although by no means a profound work, has, under Providence, exerted a remarkable influence on my life. It accidentally fell into my hands when I was about sixteen years old, and was the first book I ever read with attention. It opened to me a new world of thought and enjoyment; invested things before almost unnoticed with the highest interest; fixed my mind on the study of nature, and caused me to resolve at the time of reading it that I would immediately begin to devote my life to the acquisition of knowledge.—J. H." Each of these philosophers worked with simple instruments, mostly constructed by his own hands, and by methods so direct that he appeared to have an almost intuitive perception into the workings of nature; and each gave great care to the composition of his writings, sending his discoveries into the world clothed in simple and elegant English. Finally each loved science more than money, and his Creator more than either. There was sympathy between these men; and Henry loved to dwell on the hours that he and Bach spent in Faraday's society. I shall never forget Henry's account of his visit to King's College, London, where Faraday, Wheatstone, Daniell and he had met to try and evolve the electric spark from the thermopile. Each in turn attempted and failed. Then came Henry's turn. He succeeded, calling in the aid of his discovery of the effect of a long interpolar wire wrapped around a piece of soft iron. Faraday became as wild as a boy, and, jumping up, shouted, "Hurrah for the Yankee experiment." And Faraday and Wheatstone reciprocated the high estimation in which Henry held them. During a visit to England, not long before Wheatstone's death, he told me that Faraday and he had, after Henry's classical investigation of the induced currents of different orders, written a joint letter to the council of the Royal Society, urging that the Copley medal, "that laurel wreath of science," should be bestowed on Henry. On further consultation with members of the council it was decided to defer the honor till it would come with greater *éclat*, when Henry had continued farther his researches in electricity. Henry's removal to Washington interrupted these investigations. Wheatstone promised to give me this letter to convey to Henry as an evidence of the high appreciation which Faraday and he had for Henry's genius, but Wheatstone's untimely death prevented this. Both Faraday and he gave much thought to the philosophy of education, and in the main their ideas agreed. I may in this connection be excused from reading abstracts from a letter from Henry soon after he had received the news I had given my son his name. After a playful discusssion of the name Joseph, Jo and Josey, he says—what may be news to most of you: "I did not object to Henry as a first name; although I have been sorry that my grandfather, in coming from Scotland to this country, substituted it for Hendrie, a much less common, and, therefore, distinctive name." He then proceeds: "I hope that both his body and mind will be developed by proper training and instruction, that he may become an efficient, wise and good man. I say efficient and wise, because these two characteristics are not always united in the same person. Indeed, most of the inefficiency of the world is due to their separation. Wisdom may know what ought to be done, but it requires the aid of efficiency to accomplish the desired object. I hope that in the education of your son due attention may not only be given to the proper development of both these faculties, but also they will be cultivated in the order of nature; that is, doing before thinking; art before science. By inverting this order much injury is frequently done to a child, especially in the case of the only son of a widowed mother, in which a precocious boy becomes an insignificant man. On examination in such a case it will generally be found that the boy has never been drilled into expertness in the art of language, of arithmetic, or of spelling, of attention, perseverance and order; or, in other words, of the habits of an active and efficient life."

Henry was a man of extensive reading, and often surprised his friends by the extent and accuracy of his information, and by the original manner in which he brought his knowledge before them. Not only was he well versed in those subjects in which one might naturally suppose him proficient, but in departments of knowledge entirely distinct from that in which he gained his reputation as an original thinker. Although without a musical ear he had a nice feeling for the movement of a poem, and was fond of drawing from his retentive memory poetic quotations apt to the occasion. He was a diligent student of mental philosophy, and also took a lively interest in the progress of biological science, especially in following the recent generalization of Darwin; while the astonishing development of modern research in tracking the history of prehistoric man had for him a peculiar fascination. Yet with all his learning, reputation and influence, Henry was as modest as he was pure. One day, on opening Henry's copy of Young's Lectures on Natural Philosophy—a book which he has studied more than any other work of science—I read on the fly-leaf, written in his own hand, these words:—

> "In Nature's infinite book of secrecy
> A little I can read."—*Shakespeare.*

And did he not read a little "in Nature's infinite book of secrecy?" And did he not read that little well? May we all read our little in that book as modestly and as reverently as did Joseph Henry.

THE PHOTOPHONE.

By Alexander Graham Bell.

In bringing before you some discoveries made by Mr. Sumner Tainter and myself, which have resulted in the construction of apparatus for the production and reproduction of sound by means of light, it is necessary to explain the state of knowledge which formed the starting point of our experiments. I shall first describe the remarkable substance selenium, and the manipulations devised by various experiments: but the final result of our researches has evidenced the class of substances sensitive to light-vibrations, until we can propound the fact of such sensitiveness being a general property of all matter. We have found this property in gold, silver, platinum, iron, steel, brass, copper, zinc, lead, antimony, German silver, Jenkin's metal, Babbitt's metal, ivory, celluloid, gutta percha, hard rubber, soft vulcanized rubber, paper, parchment, wood, mica and silvered glass; and the only substances from which we have not obtained results are carbon and thin microscopic glass. We find that when a vibratory beam of light falls upon these substances they emit sounds,—the pitch of which depends upon the frequency of the vibratory change in the light. We find farther that, when we control the form or character of the light-vibration on selenium, and probably on the other substances, we control the quality of the sound and obtain all varieties of articulate speech. We can thus, without a conducting wire as in electric telephony, speak from station to station, wherever we can project a beam of light. We have not had opportunity of testing the limit to which this photophonic influence can be extended, but we have spoken to and from points 213 meters apart; and there seems no reason to doubt that the results will be obtained at whatever distance a beam of light can be flashed from one observatory to another. The necessary privacy of our experiments hitherto has alone prevented any attempts at determining the extreme distance at which this new method of vocal communication will be available. I shall now speak of selenium.

In the year 1817 Berzelius and Gottlieb Gahn made an examination of the method of preparing sulphuric acid in use at Gripsholm. During the course of this examination they observed in the acid a sediment of a partly reddish, partly clear brown color, which, under the action of the blow-pipe gave out a peculiar odor, like that attributed by Klaproth to tellurium. As tellurium was a substance of extreme rarity, Berzelius attempted its production from this deposit; but he was unable, after many experiments, to obtain further indications of its presence. He found plentiful signs of sulphur mixed with mercury, copper, zinc, iron, arsenic and lead, but no trace of tellurium. It was not in the nature of Berzelius to be disheartened by this result. In science every failure advances the boundary of knowledge as well as every success, and Berzelius felt that, if the characteristic odor that had been observed did not proceed from tellurium, it might possibly indicate the presence of some substance then unknown to the chemist. Urged on by this hope he returned with renewed ardor to his work. He collected a great quantity of the material, and submitted the whole mass to various chemical processes. He succeeded in separating successively the sulphur, the mercury, the copper, the tin and the other known substances whose presence had been indicated by his tests:—and after all these had been eliminated, there still remained